NOTICE OF CASES

OF

PLEURISY AND PNEUMONIA

IN THE

CLINICAL WARDS OF THE ROYAL INFIRMARY,

IN SUMMER 1850;

BEING THE SUBSTANCE OF TWO CLINICAL LECTURES, DELIVERED ON 7TH MAY AND 18TH JUNE.

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CASES OF PLEURISY AND PNEUMONIA.

Lecture I.—7th May.

The most remarkable peculiarity which I observe in taking charge of the clinical wards in the beginning of this summer course is the large proportion of cases of well-marked pleurisy and pneumonia, left in the wards by Dr Christison,—no less than eleven, in forty beds. This is certainly much more than is usual in the practice of the hospital. I remember being surprised, long ago when a student here, by an observation of Dr Home, senior, in his clinical experiments, that the true pneumonia (under which head he included pleurisy) is a rare disease in this hospital. This, however, agreed with my own observation when a student, and most generally since I have acted as physician here, although in some seasons we have seen much more of such inflammations than in others. In general, I have seen, or at least distinctly recognised, a much larger proportion of such cases among the out-patients of the dispensary, —seen *early* after their complaints have begun,—than in patients brought to the hospital. But at present there can be no doubt that the cases of pleurisy or pneumonia, or the (very common) combination of the two, form the majority of the pectoral complaints that you see in the wards; and, whatever be the cause of this unusual number, you have a remarkably good opportunity of studying the phenomena and progress of these complaints, and may be aided in that study more by some general observations on this class of cases, illustrating the bedside reports, than by detailing to you individual cases.

In regard to the indications by which these have been recognised (before I took charge), and are still unequivocally marked, although the inflammatory symptoms have subsided in almost all, it is worth while to fix attention on those that are most practically important—*i. e.*, those which are most uniform in their occurrence and most easily distinguished, and which, therefore,—when taken in connection with more or less of the general symptoms of fever, cough, dyspnœa, and pain of chest,—enable us most confidently to pronounce on the texture within the chest which has been affected with inflammation,—a precision of diagnosis now very generally attainable, and contrasting strongly with the candid admission of Cullen, that, in his time, diagnostic marks to distinguish the bronchitis, peripneumony, and pleurisy from each other, or even from pericarditis or carditis, during life, were not to be depended on.[1]

For this purpose we trust chiefly to what are now generally, although somewhat incorrectly, termed the *physical* symptoms of chest complaints—*i. e.*, those observed by *auscultation*, *percussion*, and *manual examination* of the thorax. Those by which pleurisy may be distinguished, generally within a very few days after the inflammation has begun, are, perhaps, the simplest and most easily recognised; particularly because, in almost every such case, the disease being confined to one side of the chest, we have the opportunity of making

[1] First Lines, sec. 334.

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comparative observations on the other; but even among them we can point out some as by much the most uniform and distinct, and, therefore, the most practically important. A young man is apt to fancy that he has acquired the most important information of this kind, when he has heard and learnt to distinguish the greatest variety of morbid sounds; but when he goes into practice he may be disappointed on finding, that some of those on which he has bestowed the most pains, seldom present themselves to his observation, and that, when he can hear them, he can draw no practical inference from them. What is most desirable is, that young men during their studies should familiarise themselves with the symptoms of this class, which are of most uniform occurrence, and form a judgment of their true import and comparative value.

In the case of pleurisy, there are three symptoms of this kind, easily observed in almost all those patients, and worth all others put together, as indicating that effusion or exudation exists in the cavity of the chest, and, if preceded or attended by febrile and inflammatory symptoms, that inflammatory exudation exists there. These are the *dulness on percussion* (greater when resulting from pleurisy than is ever observed from condensation of the lung without effusion into the sac of the pleura); *imperfect expansion* of that side of the chest on inspiration; and *suppression of the respiratory murmur*, excepting in the neighbourhood of the spine, where it is to be expected to be *tubular or bronchial*. There are three others, very distinct and satisfactory in some cases, and noted in several of those in the wards, but much less uniformly observed, requiring more peculiar conditions for their manifestation, and therefore less generally useful—viz., the modification of resonance of voice, termed *ægophony*, depending on the presence of a thin film of semi-fluid effusion between the layers of the pleura; the *friction sound* attending the acts of inspiration, and depending on the pleura pulmonalis and costalis, roughened by partial solid effusion, rubbing on one another; and the *absence of the vocal thrill*, felt by the hand applied to the chest when the patient speaks or coughs. Of these three, the last appears to me the most constant, and therefore the most valuable, particularly when going along with the dulness on percussion; as characterising the case of *fluid* effusion, separating the pleura pulmonalis and costalis at the part, and distinguishing it from the case of adhesion of these two membranes by *solid* lymph, over a condensed portion of lung;—in which case the dulness on percussion is attended with an *increase* of the vocal thrill, often very obvious in the upper part of the chest in advanced phthisis, as in several phthisical cases now in the wards. The enlargement of the side of the chest, or bulging of the intercostal spaces,—more or less general, according to the extent of the effusion,—is often very obvious and satisfactory in the advanced stage of the disease—at least of one of its forms; but is not to be looked for in the early stage, which, in all inflammatory diseases, is practically the important stage.

As to the special indications of peripneumony, whether with or without attending pleurisy, I think you will find the most important and practically useful to be that given by the *sputa—i. e.*, the appearance of tenacious and at first translucent sputa, thoroughly *stained* with blood, and thereby acquiring a considerable variety of colours in different cases, the reasons of which variety I do not pretend to know,—rusty, pink, purple, dark yellow or greenish,—all to be regarded as inflammatory; but the darker coloured, or more opaque, being generally the indication of the more advanced stage of inflammation. The *crepitous râle* is in some cases very distinct and satisfactory, but in many it is unperceived, or if perceived is faint and of short duration in the early stage of the disease, while it may be of long standing in the advanced stage, and during the convalescence; and in some it is not easily distinguished from the somewhat coarser sound of the subcrepitous râle, which may depend on conditions in the lungs, or minute branches of the bronchial, very different from inflammation. On these accounts this symptom has never seemed to me

to realize the very natural hopes of Laennec, as to its practical usefulness in detecting pneumonia, when not otherwise indicated.

The other indications that we have of peripneumony by auscultation and percussion—dulness on percussion, bronchial sound, respiration, or its complete suppression, and imperfect expansion of the affected side—are rather, in fact, what may be expected, and can hardly be distinguished from the effects of attendant pleurisy, so frequently complicating that disease.

In regard to all those physical or local symptoms of peripneumony, this observation, I am sure you will find practically important, that although we may rely much on them, particularly when grouped together, and watched in their progress, as indicating the *nature* of the disease, we cannot rely on them at all as indicating its *intensity*, its *probable tendency*, or the *extent* to which antiphlogistic remedies during it are demanded, or can be safely employed.

Take, for example, the pneumonic sputa,—they may be seen in full perfection in cases which are partial and slight, or which are attended with extreme debility, demanding stimulants; and may be absent in cases which are extensive and urgent, and will bear full evacuation. I remember having two young men—both previously in good health, and seen early—under my care at the same time, in pneumonia;—in one of whom we had the translucent sputa almost from the first, exactly resembling red currant jelly, but he was so feeble that he became faint the first time I set him up to examine his chest; and, although I thought it right to take some blood from him, he did not lose ten ounces, generally or locally. In the other, we never saw any tinge of blood in the sputa, but the other symptoms were well marked and urgent, and he bore four full general bleedings perfectly well; the blood was sizy, and the symptoms were relieved as usual after each bleeding. Both these patients recovered perfectly well; and, judging by the progress of the cases,—especially as regarded the state of the vital functions, the pulse, and breathing,—we had no reason to regret, either that the first was so little, or the second so fully bled; but if we had trusted to the sputa as indicating the nature and intensity of the disease, we should have thought that the first demanded the full antiphlogistic treatment, and the last little or none. And I have seen several, in which the peripneumonic sputa were peculiarly well marked, which I have thought it necessary to treat, from the time I first saw them (late in the disease) by stimuli only, and which have recovered remarkably well.

Again, as to the crepitous râle, I have seen many cases which seemed to me well marked by all the other symptoms, and which have done well, generally under the antiphlogistic treatment, in which I never could detect that symptom; and others, in which I was quite satisfied of its existence and extension through the lungs, while the other symptoms have either continued so mild, or been of such a nature, that no active antiphlogistic practice has appeared to be demanded or justified by that extension, and the whole symptoms have subsided without them. Particularly since I read the statements of Louis, as to the extension of the crepitous râle through the lungs, notwithstanding full bleeding, by which the pulse had been brought down, —from which he inferred that the idea of the power of blood-letting over inflammation had been a mistake,—I have attended to this point, and satisfied myself that he was quite correct as to the fact, but wrong as to the inference—I mean as a general principle; because, in these circumstances, when the pulse and breathing have decidedly improved, although the râle may indicate that the inflammation continues and extends for a time, we may be very confident that it will subside without further treatment. The reason probably is, that the inflammatory exudation, although extending, is modified in character, and so attenuated as to be easily re-absorbed,—so that no complete obstruction, or permanent disorganisation of the pulmonary substance, to any injurious extent, is effected. In these circumstances, all that is required is, to watch the patient carefully, and revert immediately to the bleeding, or other

antiphlogistic treatment, if the breathing becomes again more oppressed, and the pulse more frequent or *firmer;* under the conviction, given by experience, that the crepitous râle extending through the lungs in a case of recent illness, and in connection with a frequent and full pulse, and a laboured state of the breathing, implies much danger, which may be averted by such treatment; but that the same râle extending through the lungs, in connection with a more natural pulse, and an easy state of the breathing, or with dyspnœa, but a feeble or soft pulse, and damp livid skin, either implies no danger, and demands no active treatment, or is to be counteracted by very different remedies. And I may add, that this is exactly the line of conduct which our predecessors would have advised in those circumstances, before the crepitous râle was heard of, although they could not have spoken with so much precision as we can, of the nature of the changes then going on in the lungs.

I remember a case of a gentleman advanced in life, whom I saw some years ago, affected with urgent pneumonia, who was bled seven times, and the last bleeding seemed to me as clearly demanded (as indeed it was anxiously wished for by the patient), and as clearly effectual in relieving the dyspnœa, as the first; but the crepitous râle distinctly extended itself through the whole of the affected lung, and maintained itself, becoming only very gradually coarser, for a fortnight thereafter,—the convalescence going on, in all other respects, perfectly well. Last year I saw this gentleman in another attack of pneumonia, with the febrile symptoms and the characteristic sputa, and found the crepitous râle re-established over the greater part of the same lung as formerly; but the general symptoms were on this occasion, at least from the time I saw him, so typhoid, and the pulse so soft, that it was thought best, not only to dispense with bleeding entirely, but after a short time to give stimulants (of course carefully watching their effects) as in typhoid fever; I had the pleasure of seeing all the symptoms again gradually subside under that treatment, and that gentleman, although now past 70, is again in the habit of walking several miles daily. Now in that case, if we had regarded the crepitous râle as an index to the intensity or danger of the inflammation, or as a warrant for blood-letting, we should have thought active treatment required in the convalescence from the first attack, when no remedies were really required, and when it would pretty certainly have been dangerous; and should have resorted to bleeding in the last attack, when it would, to say the least, have been unnecessary. But all this does not prove, either that the bleedings, which appeared so signally useful in the early part of the first attack, were not really useful, or that the inflammation might not have been most beneficially combated by that remedy in the second attack likewise, provided it had been used within the first 36 or 48 hours. And in drawing any such negative inference, we should have opposed ourselves, without sufficient reason, to the belief as to the power of blood-letting over the early stage of inflammation, which has prevailed from the earliest age of medicine, and which is confirmed to most of us by numerous cases, carefully watched, and even by experience in our own persons.

If we regard the different indications that we now have—not strictly speaking of inflammation, but of *inflammatory effusions* in the chest—as I think we ought to do, as important evidence of the nature, but not of the degree of intensity, nor of the stage, nor proper treatment, of the diseased action there existing, we shall have less difficulty than we otherwise might have had, in reconciling such facts as we have now before us, with the general principles as to inflammatory diseases, which have been handed down to us from our predecessors, and on which we still confidently rely, although aware that they require modification and limitation from the progress of knowledge.

Thus, you may think it strange, that we should have so many unequivocal examples of these inflammatory diseases, without a single opportunity, in my colleague's practice or my own, of seeing the effect of general bleeding—which we nevertheless both regard, now as formerly, as being the most powerful remedy in this disease, at least in the form of it most generally described, and

with which we are all familiar ; and this consideration suggests several reflections which I think important.

1. You will observe that several of these cases, known to us with almost absolute certainty as effusions consequent on inflammation in the chest, would not have been recognised in former times at all, under their true denomination ; and this no doubt is part of the reason why these inflammations were thought so rare. Some of the cases now before us would have been called catarrh, some pleurodyne or rheumatic stitch, some incipient phthisis, some would have had no more definite name assigned to them than broken constitutions, threatened with hydrothorax ; or if, by tracing the whole history of the cases, their real nature had been made known, most of them would have been termed, and indeed correctly termed, the *sequelæ* of pleurisy or pneumonia, not the inflammations themselves. They would, therefore, have been regarded as, either by their nature or the stage at which they were seen, distinct from cases of true inflammation within the chest, and would not have been thought to demand any such active practice as is usually associated with those names. Nor would the uncertainty which undoubtedly would have then existed as to their real nature, have had any such injurious practical effects as you might suppose, because these patients are all now in such an advanced stage of disease as admits, and would have been long ago known to admit, of no active depleting practice ; and the chief benefit to be expected in them, must be from the antiphlogistic regimen,—particularly as regards rest, avoiding excitement, exertion, exposure to cold or wet, intemperance, and all other irritations, and from the use of a diet suited to their condition and the stage of their diseases,—and so giving fair play to the provisions of nature for the restoration of health ; all which would have been enjoined by a prudent practitioner, notwithstanding his uncertainty as to the exact nature of their complaints. The more accurate discrimination of such cases, which has been attained of late years, is satisfactory to our minds, and is often valuable with a view to prognosis, and occasionally it suggests useful practice, but is not so frequently available for the good of patients as you might suppose.

2. But you may further very naturally ask, whatever be the stage of disease in which those cases come under observation, if it be admitted, as I believe it may, that they have all been truly inflammatory in their commencement, how comes it, that none of them have been bled at the arm, either out of the hospital or in it, and yet almost all appear in a fair way of recovery ? There is a poor woman, M'Govan, brought several miles from the country some days ago, in the seventh day of disease, which seems to have been attended from the first with typhoid symptoms, as she was sent in as a case of fever, but whose case I regard as simply pneumonia, affecting almost the whole of the left lung, exhibiting all the usual local symptoms, with a pulse from 120 to 140, always soft and feeble, a cool skin, clammy sweats, livid lips, a dry tongue, and such marks of debility attending the dyspnœa as I have never seen recovered from, at this stage of the disease, under any other treatment than that which we have here adopted, viz., the strictly stimulating. This case we must expect to be fatal,[1] but all the other ten may be expected to recover, with some injury to the play of the lungs, and therefore shortness of breath, which will gradually diminish if there is no fresh attack,—although none of them have been bled. Such observations have been repeatedly made of late years, and have been thought by some to justify the conclusions : that these inflammatory diseases are not so dangerous as they have been represented; and that the active practice recommended for them in systematic works is quite unnecessary. These are points deserving grave consideration. Certainly it must be admitted, that since we have had the means of detecting effusions consequent on inflammation in the different parts of the chest,

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[1] This woman died on the fifteenth day of her disease, but unfortunately permission to examine the body was refused.

we have recognised inflammations there more frequently, and in cases apparently requiring less active treatment, and doing well under a less active treatment, than we could have expected; and in many such cases we can satisfactorily observe—and I have no doubt shall be able in some of these cases in the wards to observe—the important and essential difference between inflammatory exudation, and those which constitute other organic diseases, tubercular, or fatty, or granular, or malignant deposits,—viz. that the former, when the inflammation has subsided, are so much more liable to absorption, than any of the latter,—that being the main agent in the natural cure of all lesions of structure produced by inflammation, such as you will here have the opportunity of observing, under which an organ is indeed seldom restored to its perfectly natural condition, but is very often permitted to resume the natural exercise of its functions in ordinary circumstances.

On this point—the efficacy of the provisions of nature for the cure of inflammatory disease—we have important facts in the experience of *homœopathic* practitioners,—for I presume we are agreed on this, that the treatment by infinitisimally small doses of a variety of medicines, all thought to be specifics, is in fact treatment by *placebos*, or is another name for what is usually called the *expectant* practice, admitted by all to be, in *some* circumstances, the best of all kinds of practice. We must admit, I think, that this practice has appeared, on fair inquiry, to be more frequently successful in inflammatory diseases than could have been expected—*i. e.*, the practitioners who have thought themselves justified, by that theory, in trusting more than we do to the provisions of nature, aided only by regimen, for the cure of such diseases, have had fewer deaths and better recoveries than we should have expected.

One reason for this you can anticipate from what I have already said. This theory has come into vogue only since the means of detecting the local indications of inflammation within the chest have been so much improved, as to make us sure of its existenee, and of its being of some extent, in many cases where our predecessors would have overlooked, or could only have suspected it. In fact, you will remember that the terms pleurisy, pneumonia, &c., as defined by Cullen and others, have a *nosological*, not a strictly *pathological*, meaning; they are not applicable strictly to inflammatory effusions, but to those effusions taking place in connection with a certain group of symptoms,—pain, deranged function of the affected parts, and inflammatory fever. We now know that exactly similar effusions or exudations take place in what we call the more chronic or even latent forms of the disease (most common, I believe, in persons of the lower ranks of life, in whom all vital action is somewhat languid), with slight and insidious symptoms in their commencement; and that they take place also, in some cases (especially, as some have stated, in the upper part of the lungs), with a febrile re-action of the typhoid rather than the inflammatory character; and, of course, it is only by experience we can learn how far the antiphlogistic treatment—judged to be so useful and important in the strictly acute cases attended with inflammatory fever—is applicable to those more obscure and insidious forms of the same diseases.

Such cases as we have before us are not so important as you might at first suppose, in enabling us to judge of this question. Certainly, you are not entitled to infer, from such observations as these, that blood-letting has not the power which is usually assigned to it, of checking inflammation in its early stage, simply because you have not seen these cases in their early stage, and cannot, therefore, tell how far they could then have been recognised by their symptoms, nor what would have been the effects of any remedies on them. The first inference which I think it most important to impress on you is, that hospital practice seldom gives a fair opportunity of judging of the power of such remedies over inflammation; and this is quite in accordance with what I have very frequently observed formerly, particularly in comparing hospital and dispensary practice. I have little doubt that if all these eleven cases had been seen within the first forty-eight hours of their existence, some of them at least might have

been recognised, and if assiduously treated by general and local bleeding might have been so far controlled, that the effusions we now perceive might have been either altogether prevented, or so modified as to be hardly perceptible, and the patients have been restored to health by this time.

I have often quoted, in illustration of this remark on hospital practice, a case narrated by the late Dr Jackson, a young American physician, who was a very zealous and diligent student at one of the hospitals at Paris, and on which he dwelt with delight, as "the most superb he had ever seen." The case was one illustrating merely what most of us regard in this country as one of the simplest and most elementary principles in medicine—the power of blood-letting over incipient pneumonia; but Dr Jackson says, that although it was the twenty-first case of pneumonia that he had watched with the utmost care in that hospital, it was the first in which he had seen the power of this remedy distinctly exemplified; all the symptoms, local as well as general, although quite distinct and following their usual course, having in this case disappeared by the sixth day; but then he adds, that it was the first case he had seen, in which the treatment was begun within the first twenty-four hours. The inference from such a fact, as to this important deficiency in the practical information to be derived from hospital practice, is well worth your consideration.

But this leads me to take notice of another statement made by the homœopathic practitioners, which is certainly more staggering, viz., that under their plan of treatment the convalescence from inflammatory diseases is more rapid than under the depleting plan. This, they say, they can show statistically; and although I place little reliance on statistics in such an inquiry (on account of the numerous fallacies to which they are liable), yet I know that this observation has been made by intelligent and unprejudiced witnesses of this treatment—*e. g.*, by Dr Balfour, who saw it in Vienna; and a little reflection will enable us so far to understand it. For all must allow that, after full and repeated bleedings, whether for inflammatory or other diseases, convalescence is necessarily slow; and farther, that relapse is easily produced, inflammation being always easily excited in a weakened constitution.

But then you will observe, that a rapid recovery, particularly as regards the general symptoms of disease, up to a certain point of *general* health and strength, is not necessarily a perfect recovery; and that a slow convalescence, although attended with the inconvenience I have mentioned, may in many cases be the most complete, as in some, we are convinced, that it is the only possible kind of convalescence, of which the case admits. What we apprehend from a pneumonia, met by what we believe to be inadequate treatment, is, not pure exhaustion of the vital power, but such an amount of inflammatory effusion as may either cause rapid death by asphyxia, or lay the foundation of chronic and incurable organic lesion of the lungs, the precise nature of which may vary according to the constitution of patients. This local injury consequent on inflammation is quite compatible with a rapid recovery of a certain degree of health and strength—such as to enable a patient, for example, willingly to leave an hospital. The only perfectly fair way of determining this question by statistics would be, to have a certain number of patients treated from the beginning, in inflammatory diseases, by practitioners accustomed to the use of blood-letting in such diseases, and with the symptoms which they have been accustomed to regard as indicating it, by the expectant practice only; and then an equal number treated by the same practitioners, likewise from the commencement, by the depleting remedies; and farther, we must have perfect confidence, if not in the judgment, at least in the good faith, of the practitioners using (and not abusing) these active remedies. To such a trial I shall only say, that I should not wish myself, nor advise any of my friends, to be a party; and the moral reasons which make me say so, must, I think, for ever prevent any such statistical experiment being really tried by any trust-worthy practitioner.

What I have stated is enough to show that some of these cases, occurring in

former times, would have been differently named, although not, as to essentials, differently treated ; and this, I think, may go far to explain the fact which I stated as presenting a difficulty,—the number of cases of thoracic inflammation now going on favourably, without affording you an illustration of the use of general blood-letting. But I must confess, that I do not think this explanation sufficient. I am strongly inclined to believe, from reflection on many other cases as well as these, that there has been a gradual change in the usual form and character of those inflammations, as occurring in the inhabitants of this country—and that, even although seen from the earliest period at which they can be recognised, they do not, in general, present the same intensity of local symptoms, nor the same amount of febrile re-action, as used to attend similar diseased actions in the same parts in former times ; that the fever attending them is more frequently either slight, or in several respects of a truly typhoid character ; and therefore, that they do not furnish the same indications for blood-letting, and do not admit of the same amount of depletion, without dangerous depression of strength.

On this point I do not speak with absolute certainty, but I can give you what I may call statistical evidence on it ; for it not only has appeared to myself that there is a change in the usual character of inflammatory diseases within my recollection, but I have put the question to many experienced practitioners in different parts of Scotland and the north of England, and in every instance but one I have been assured, that the result of their observation agreed with my own, and that, judging by the same rules as formerly as to the cases requiring it, they have much fewer occasions to use the lancet for inflammatory complaints now, than they had thirty or even twenty years ago,—or than might be expected from the writings of Cullen, or even of Abercrombie, or from the well-remembered precepts of Gregory. Of course it may be said, that this is only to be ascribed to the gradually increasing caution of advancing years ; but when I add, that the faith of all the gentlemen to whom I allude, in blood-letting for the cure of well marked and violent inflammation, is unshaken, and as they believe confirmed by experience, and that they think, as I do, that cases still occasionally present themselves in which it appears as distinctly demanded, and as clearly useful, as formerly ; but that, in general, the symptoms on which we rely, as guiding us in its use, are either so slight as not to demand it, or of such a character as to limit or even contra-indicate it, before it has been carried to any considerable extent,—you may probably think this general inference from extensive experience deserving of attention.

We have instances before us which illustrate what I now say. Take, for example, the case of Michael Joyce, a lad of 18, of rather full habit, admitted into No. 1 the day before I took charge. We have evidence in him, certainly of pleurisy, probably of pleuro-peripneumony, affecting at the time of admission nearly half the right side of his chest, and the *lower* half, *i. e.* the part most liable to acute inflammation. We have dulness on percussion, impaired expansion, bronchial or suppressed respiration, friction sound, and muco-purulent sputa ; and we know that up to the fifth day before admission he had been working in the fields, and then been disabled by rigors, dyspnœa, and pain of side. I am confident that, in former times, from this history and these local symptoms, we should have safely anticipated acute inflammatory fever, and an opportunity for full blood-letting ; unless, indeed, the disease had made too much progress to admit of it ; but in this case we have had the pulse 70 on admission, never above 80, always soft, the skin always cool, the pain and dyspnœa never urgent. He had twelve leeches and a blister, ordered by Dr Christison, with such relief that I have only thought it necessary to order eight more leeches, and already we have indications of beginning re-absorption of the effusion.[1]

[1] This lad had two returns of pain of side, with slight febrile symptoms, and extension of the marks of pleuritic effusion over nearly the whole of that side, for which

You are aware that Sydenham and other authors have written much on what they term the atmospheric constitution of different seasons, not only as affecting epidemics, but as modifying the usual character of sporadic diseases; and although there are many fanciful notions and minute distinctions, not now to be recognised, in those writings, I believe all reflecting practitioners of some standing will allow, that there is a foundation in nature for such observations. And I beg you to observe, that although Sydenham speaks of atmospheric constitution, he expressly says that he cannot ascribe these varieties of disease to *known* qualities of the atmosphere, having observed the same characters of certain diseases to prevail throughout whole years, and when the temperature varied from intense heat to the greatest cold within the memory of man. Therefore, he says, these varieties of disease must be ascribed to "*occult* qualities of the atmosphere,"[1] which is the same thing as saying, that he did not know what produces them; and I apprehend we must acquiesce in this conclusion likewise. But although we cannot assign the cause, we are quite certain of the fact, of such variations in the usual character of the *epidemic* diseases—as I believe every practitioner is aware, who saw as much as I did of the epidemic fevers prevailing here between 1815 and 1825, and can compare his recollection of those fevers with observations of those which have constituted the three great epidemics that have since prevailed here—commencing in 1826, 1836, and 1847; the typhoid character of all these epidemics contrasting strongly with the inflammatory character, and the local inflammatory symptoms which constituted the chief danger of the fevers of the preceding ten or fifteen years—such as those described by the late Dr Duncan and Dr Welsh in this town, and by Dr Bateman in London. And I may add to this another observation, of which I am equally certain, and which I have long thought important, as unequivocally showing that such variations in epidemics are not to be explained by season or sensible variations of the atmosphere—viz., that the inflammatory epidemic of continued fever observed from 1815 to 1818, was coexistent with a more typhoid form of measles than we have ever seen since; —the eruption in the bad cases of that epidemic having been late, livid, partial, rapidly fading, and attended and followed by stupor, retching, dry tongue, &c.—such cases being easily depressed by evacuations, and obviously benefited by stimulants, and, when the dyspnœa was not urgent, very often recovering under their use.

Now, if epidemic diseases are thus subject to an influence, unknown to us except by its effects, which modifies their symptoms and tendency so remarkably at different times, there is nothing unwarrantable in the supposition that the usual tendency of *sporadic* diseases likewise,—and particularly the febrile reaction excited by those local changes to which we give the name of inflammation,—are liable to similar variations, demanding much modification of the treatment; and I need not say, that if this principle is true, there is no other more worthy of your attention and recollection.

Next, as to the practice to be employed, in addition to the antiphlogistic regimen, in cases of those inflammations which, either on account of the stage in which they are seen, or the general symptoms they present, are not judged proper for general blood-letting, I need hardly say that local bleedings and blisters, or other counter-irritants, are the next remedies to be thought of; and you have had, and probably will have, various opportunities of observing their good effects. But it is easy to perceive that both may be too much relied on, or carried too far,—the former because they may so far depress the strength, and the latter because they may cause so much pain and irritation, as to interrupt and retard, rather than promote, the gradual progress of the natural

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one leeching and two blisters were again directed; but the pulse never reached 84, and was never full or firm; and he was free from complaint, and almost free from indications of any disease in the chest before the 15th of June.

[1] 1st Epistle on Epidemic Diseases, § 5.

changes on which recovery in these cases must mainly depend. The same observation applies to the antimonial solution, which, given in the manner recommended by Laennec, *i.e.*, *altera quaque hora* (although in much smaller doses), I have been accustomed to regard as the most powerful and manageable auxiliary to, and sometimes substitute for, the loss of blood in these inflammations,—and even to the still smaller doses of antimony or ipecacuan with morphia, which are usually the most effectual palliatives for the irritating cough, but retard the recovery of the stomach, in these complaints.

The next question is as to the use of mercury, most generally in the form of calomel and opium, which many practitioners regard as of signal use in pneumonia, and more especially in pleurisy. I must confess that I have so often given this, or seen it given, and watched its effect as carefully as I could, at the time when the mouth became affected (which is the only time when you can be sure of distinguishing the specific effect of this remedy), without being able to perceive any advantage from it,—nay, in some cases where the irritation excited by it has seemed to me manifestly hurtful,—that I have no such faith in it as many profess, and think it necessary to be cautious in using it, and watch carefully for any of its untoward effects,—particularly in hospital patients, often of feeble constitution, and soon to be exposed again to cold and hardships. But in the pleuritic cases, when there is no peculiarity of constitution to contraindicate it, I generally think it right to make a cautious trial of it.

In the cases of peripneumony in the advanced stage, or with the typhoid symptoms well marked and urgent, I am certain that by far the most important remedies are the stimulants, in small doses, carefully watched and gradually increased,—wine, brandy, and different preparations of ammonia and æther. This kind of practice is more frequently required in the advanced stage of mere bronchitis, both acute and chronic; but I have seen several of peripneumony, in which it seemed as clearly demanded, and as decidedly useful, as in any case either of bronchitis, or even of typhus itself, in which I have ever used it; and although the case of M'Govan seems too far advanced,—*i. e.*, the effusion probably too great, and the febrile symptoms attending it of too intense a character, to be benefited by this or any other remedy,—yet it is very possible that we may have other opportunities of witnessing its successful employment.

Lecture II.—18*th June.*

You have had opportunities, in the last six weeks, of watching the progress, not only of the cases of pleurisy and pneumonia formerly mentioned (all of which, except the fatal one of M'Govan, have gone on favourably), but likewise of two cases of well marked pleurisy—sub-acute and chronic, and one case of well marked pleuro-peripneumony—which have been admitted since; besides several cases of decided phthisis, and several of bronchitis with asthma, the diagnosis of which has been easy, excepting one case of that kind (Mrs Kerr), in which there has been pretty violent hæmoptysis, and strong suspicion of a phthisical cavity in the upper part of the left lung. I say suspicion, because there is no doubt of general and habitual bronchitis with asthmatic paroxysms in this case, and where these exist it is not uncommon, in the lower ranks, to have repeated attacks of hæmoptysis which never pass into phthisis,—unconnected therefore, I presume, with tubercular deposits, but probably connected with apoplexy of the lungs, as in many cases of disease of the heart, particularly of the mitral valves. And although the sounds in the upper part of the left side in this woman are not to be distinguished from the resonance of voice, bronchial respiration, and cavernous râles, of tubercular condensation and ulceration, yet we know that in severe cases of bronchitis affecting this part, especially if there be any dilatation of the bronchi,—in connection with condensation of lungs from any other cause,—the sounds elicited may be for a time exactly those which I have

mentioned. From this cause we know that some cases of inveterate bronchitis and asthma have been mistaken, even by practitioners thoroughly experienced in the use of the stethoscope, for cases of *phthisis confirmata.*

One of the cases of pleurisy recently admitted is a woman named Hutchinson, æt. 35, admitted 31st May. This case is chiefly remarkable for the extent of pleuritic effusion, shown distinctly by the usual marks, occupying, from the time of admission, the greater part of the right side of the chest, and obviously displacing the heart towards the left axilla (though without protuberance of the intercostal spaces), while the pulse is little affected, and her breathing, when she is at rest, apparently easy. The disease in her, nevertheless, appears to be of short duration; and at the time of admission, she had ℥xij of blood taken, which was sizy (though without any marked aggregation of the coagulum), leeches and a blister, with relief to the pain and sense of tightness in the chest. Since then, the only remarkable fact presented by this case has been, a return of the pain, followed by a decided extension of the pleuritic effusion (as you will see by the Reports) at the time when her mouth was distinctly affected by mercury. The very same thing happened in the case of the lad Joyce, formerly mentioned; and I regard these two cases, therefore, as favouring the incredulity which I formerly expressed as to the utility, at least as to the importance, of mercury, given so as to affect the system in cases of this kind.

Of course you will understand that when we see inflammatory symptoms easily produced, or reproduced, in a person under the influence of mercury, we are always apprehensive of these becoming speedily connected, in one way or another—we need not at present inquire how—with tubercular deposits and scrofulous disease; knowing as we do, that in this climate such disease is a very common consequence, in certain constitutions, of the agency of mercury, especially if the patient is exposed to cold and wet during or immediately after its effect on the system. In this case of chronic pleurisy it might be supposed that this danger is merely imaginary, because it has been stated that the complication of chronic pleurisy with phthisis is very rare. Dr Blakiston, in particular, speaking from the result of extensive hospital practice, says he had traced the history of above 50 cases of chronic pleurisy, without finding that any of them terminated in phthisis. This observation is well worth attention; and certainly I have seen many cases, where the general symptoms were so much like phthisis that they have been thought decided examples of that disease, but in which chronic pleurisy has been detected, either during life or after death, and where no tubercular deposit ever took place; and I believe you will hardly ever see any very large pleuritic purulent effusion in persons in whom, at the same time, tubercles are in an active state. But I have seen quite enough to convince me that the complication of the two diseases often happens in two ways. 1st, That pleuritic effusion, of various degree, extending sometimes over much of one side of the chest, takes place as an intercurrent inflammation in the course of phthisis—sometimes at an early period of the disease, sometimes late—and may either be re-absorbed, or remain till death, without materially altering the course of the disease; and 2d, that after pleuritic effusion has been chiefly or entirely re-absorbed, the patient, although previously of strong habit, may be left in a state of debility, in which he may readily pass, from slight causes, into tubercular disease. And this last fact in particular is sufficient to show us, that in treating chronic pleurisy by mercury, we ought always to have before our eyes the danger of the patient falling into phthisis, if exposed, even slightly, to its usual exciting causes, during, or for some time after, the mercurial course.

The case of George Sutherland (æt. 29), admitted 22d May, is the only distinct one we have seen of pleuritic effusion (on the left side), besides presenting the usual indications formerly detailed, so distending that side of the chest as to cause bulging of the intercostal spaces, and such displacement of the heart, that its pulsations could be most distinctly felt, and even seen, to the right of the sternum; and the liver, somewhat swelled and tender (as it most generally

is in such cases, especially in young persons), was pretty certainly forced downwards, and was felt dull on percussion $1\frac{1}{2}$ inch below the margin of the ribs. It is important to observe that in this case, as in almost all that I have seen, where the effusion ultimately became very great, it never was attended by acute symptoms. His complaint commenced in August last, with a fit of hæmoptysis (and he has now somewhat suspicious sounds at the apex of the *right* lung); since then he says he has suffered repeatedly a good deal from cough and dyspnœa, but seems never to have been confined to bed, and says, positively, he never had pain of side, and always could lie on either side. His pulse, on admission, was 88, and he suffered a good deal at times from cough and dyspnœa.

Now in this case, there was no such urgent dyspnœa, nor appearance of immediate danger, as I should have thought requisite, some years ago, to justify the paracentesis. But since I have been acquainted with the experience of Dr Hughes, and Mr Cock of Guy's Hospital, as to this operation, and since I have known it performed by Mr Syme—particularly in one case which I recommended to him some years since, and in which it has been repeated many times—with the simple precaution of using *a very small canula, not* $\frac{1}{10}$*th of an inch in diameter*, so as to allow the fluid to dribble off very slowly, and to withdraw the little tube and close the orifice as soon as the fluid begins to drop, so as to provide against the entrance of air into the chest, and to be sure that the fluid taken off is only as much as had *distended* that side of the chest and compressed and dislodged the heart and lungs,—I have been so convinced of the safety of the operation itself, when skilfully performed on this plan, that I have thought it sufficient to justify our advising it, to find the intercostal spaces bulging out, the heart manifestly displaced, and palpitation and dyspnœa easily excited by any exertion. At least, if these phenomena occur in a case which has been slowly making progress, and is apparently unaffected by a fair trial of the usual internal remedies, it may always be feared, that even if absorption does spontaneously take place, it will be so slow, and attended with such debility, and probably hectic fever, as to imply much danger. And although I have known various cases of very good recovery, after such collections of matter within the chest have made their escape by ulceration, either externally or through the lungs, yet the event of any case in which such ulceration take places, must be regarded as very precarious; whereas if the fluid distending one side of the chest, and threatening such mischief, can be safely drawn off, it may be hoped that the absorption of what remains may take place more rapidly, and the danger attending any such ulceration be averted.

Accordingly, after consultation with Mr Syme, and with the staff-surgeon here (as this man is a soldier on sick leave), the left side of the thorax was tapped by Mr Syme, in the way above described, between the fifth and sixth rib, about half-way between the middle of the sternum and spine, and sixty-six ounces of sero-purulent fluid drawn off, in forty minutes. Since the aperture was closed, there has not been a drop of fluid evacuated, nor any complaint of pain or dyspnœa, nor febrile symptom; but after the operation, the left side of the chest still measured $1\frac{1}{10}$ inch more, from the spine to the middle of the sternum, by a line drawn horizontally across the nipples, than the right; and the sound of the heart, although now distinctly heard to the left of the sternum, was still loudest immediately below the centre of that bone. He had been taking a Plummer's pill at night, and gr. 1 ss. of iodide of potassium three times a day before the operation, and this was continued in the hope that the requisite absorption might be thus promoted.

[On the 4th July, *i. e.* thirty days after the operation, the two sides of the chest measured at the same line as before, were exactly equal, and in the upper part of the left side of the chest the sound on percussion was somewhat clearer and the sound of respiration distinctly more natural; but in other parts there was still much dulness on percussion, and the sound of respiration only tubular; and the sound of the heart was still heard loudest beneath the middle of

the sternum. His pulse was now natural, tongue clean, appetite good, the swelling of the liver diminished, and the tenderness there gone; he had no cough or dyspnœa, and could walk several miles without suffering in any way. His mouth was somewhat sore, and his breath tainted, but he had no salivation from the calomel. He was then dismissed, by desire, with cautions as to his conduct,—and certainly with a very fair prospect of the absorption requisite to a complete recovery going on satisfactorily; and we have good reason to believe that this process has been materially expedited by the operation, and perhaps somewhat by the treatment here adopted.]

The case of J. Gayner (æt. 25), admitted 7th June, on the fifth day of pneumonia, with pleurisy of the upper half of the left lung, is a still more remarkable one, particularly as illustrating what I have stated of the typhoid nature of the febrile reaction, frequently observed as attending such inflammations at present, and the modification of practice thereby required. This man had been much exposed to cold, wet, and fatigue in working at a breakwater at Leith, particularly during the night, and had been seized with rigors on leaving work on the 3d instant, the fifth day before admission; he had had much cough, with febrile symptoms, and great weakness from that time, and his sputa had been stained with blood before admission; but at the time of admission there were no such sputa, and not much cough, although the cough became urgent and the sputa quite characteristic very soon after; his pulse was 100, full and soft—his aspect so much that of typhus, that I thought the diagnosis doubtful, and the indications in the left side of the chest were *slight* dulness on percussion in the upper part of the left side, and a crepitous râle heard distinctly on full inspiration, and attended with slight resonance of voice, in different parts of that region, but not extending lower than the inferior angle of the scapula. He had twelve leeches that forenoon, and as his breathing seemed more oppressed,—and as I found he had passed through typhus fever in the hospital two years before, making him less liable to another severe attack of that kind,—he was directed to be bled at the arm to ℥xij in the evening, but fainted after ℥vi had been taken. After the bleeding his pulse was 84, and weak. This blood showed a distinct buffy coat, but the coagulum was not firm, being easily torn by its own weight when lifted on a pin. After this time he has had no more bleeding, but two more leechings, making thirty-two leeches in all; he had two blisters to the side, and was put one day on the use of the antimonial solution in doses of only $\frac{1}{6}$ gr. every two hours, which caused, however, sickness and vomiting, and so much general depression, that it was very properly stopped in my absence; and since then his symptoms, as you will see by the Reports, have distinctly exemplified the following points, illustrating several of the statements I formerly made:—

1. The fever has had, all along, the typhoid form; the expression of his countenance has been exactly like that of the man lying opposite to him, who has had unequivocal typhus—he has had the same drowsiness and indifference to surrounding objects—his pulse has always been soft, even small, his tongue dry, first in the centre, afterwards throughout, his lips incrusted—his skin has never been hot, generally moist, and he has shown more indications of general weakness than many of those patients in pneumonia whom I have seen, on former occasions, sent into the hospital as cases of fever; for many of these have borne evacuations better than was anticipated, and improved in strength rapidly after repeated, although moderate, bleeding; whereas this man bore evacuations ill, and had so obvious depression of strength, that he has been allowed ℥iv of wine daily since the 10th instant, and a mixture containing syrup of squill, with spiritus ammoniæ aromaticus; and there has been neither heat nor flushing, nor increase of cough, or of pain after it, and his pulse has come down from 96 to 84 and 78 within three days after this was allowed. The last leeching and blistering, however, have been ordered since this allowance of wine was begun.

2. You will observe that, even after most of the evacuations I have mentioned, there has been a gradual extension of the inflammatory effusions, just

as described by Louis. The sound on percussion, almost natural at first, became quite distinctly dull in the upper part of the left side, both anteriorly and posteriorly, and this was attended with absence of the vocal thrill; and the sound of respiration became faint, and mixed with somewhat of the crepitous râle gradually changing to subcrepitous, almost to the base of the lung, while the sputa remained distinctly pneumonic, even after the last leeching and blistering. By these marks we were well assured of the extension of inflammatory exudation both in the lungs and on the pleura; and, by the way, I may remark that these unequivocal marks of *pleuritic* effusion are nearly enough of themselves to set aside the idea of typhus or epidemic fever, during the continuance of which I am sure you will find that true pleurisy hardly ever exists, although particular forms of peripneumony and bronchitis are common.

3. You might observe here, that, *co-existent with this extension of the inflammatory exudation, there was abatement of the general symptoms*, particularly as to the essential points, the state of the pulse and breathing, the pulse coming down below 80, and the breathing, never in this case marked as above 30 in the minute, becoming less frequent and easier, while yet extension of the effusions was going on. I think this implies that the inflammation, although extending, was of slight intensity, and this is confirmed by the improvement which has since taken place, and I hope will be by that which, unless I am much mistaken, you will find is to follow. This we cannot ascribe to the use of the antimony, for it was discontinued after two small doses. Neither do I think we can ascribe it to the use of calomel and opium, for, although he took this in small doses (12 gr. of calomel in all) on the 11th and 12th, this was discontinued on account of diarrhœa, with very slight soreness of mouth; and besides, the improvement as to the pulse and breathing had begun before the mercury was used. The abatement of the symptoms may have been referable only to mildness of the inflammatory attack; but judging by the local symptoms, I can certainly say this, that many cases are fatal in which the portion of lung obviously disqualified for its functions is less than that which appeared here to be involved. I think it, therefore, reasonable to suppose, that as it certainly was the object, so it was the effect, of the evacuations practised in the first three days that he was in the house (although much smaller than in many cases, where the febrile reaction is stronger, is necessary for this purpose), so to modify the inflammation, that the effusions consequent on it, although extending considerably thereafter, did not produce such condensation of the lung as they otherwise would; and were liable, subsequently, to the rapid absorption which I think is already in progress, and of which, unless I am much mistaken, you will see further evidence by watching this case.

If this case had occurred to me twenty-five years ago, I should have practised the same evacuations on his admission, and after that, being warned by the typhoid nature of the symptoms, and further by the diminished frequency of his pulse and improved breathing, to suspend such treatment, I should, without putting my ear to his chest, merely have watched and prescribed for the state of his general symptoms; and finding them gradually to improve under the cautious use of wine and ammonia, should have regarded the case as an illustration of one of the best established and most easily understood principles in medicine—the power of blood-letting—suited in its extent to the state of the general symptoms,—to check inflammatory effusion and its effects on the lungs, even as late as the fifth day from the attack; although affording less striking evidence than we should have had from using the same means on the second or third day,—and although used in a case where the general symptoms were such as to demand the use of stimuli almost immediately after. The information which we now have enables us to speak more precisely as to the changes going on in the lungs; and requires us to alter our language so far, as to use the term modify or moderate, instead of check or subdue, the inflammation, as applicable to the power of blood-letting on the fifth day of the disease; but this is a change of language which a little reflection would

have shown to be right, even independently of such more precise information, and when the whole history of the case is fairly considered, I think the practical inference is not materially altered.

4. It is probably more practically important to observe, that in this case, as in many others which I have seen of late years, where the pulse has been soft and the febrile symptoms typhoid, there has been a gradual abatement of all the symptoms of the peripneumony—first of the general, and then of the local—under the use of wine and ammonia, begun as early as the seventh day of the disease, while the pneumonic sputa were in full perfection, and the dulness on percussion and crepitous râle *still extending* over the side; and that in this case, just as in that of typhus fever complicated with pectoral inflammation, there is no inconsistency in using the local bleeding and blistering for relief of the cough, pain of chest, and dyspnœa, at the same time that these stimuli are cautiously given to maintain the heart's action and promote the expectoration; in short, that the state of the general symptoms—the degree and the kind of the febrile reaction that is excited by any inflammation in these parts—known to be remarkably various in connection with nearly similar states of inflammation, in different individuals,—must be our guide in the administration of general remedies. This conclusion is quite in accordance with what has been stated as to such cases of typhoid pneumonia by Dr Stokes and others; but it is of so much importance to know, not only that such typhoid cases, requiring early modification of the practice, may occur, but that they are especially to be looked for at certain times and places, that it is right to lose no opportunity of impressing this principle upon you.[1]

During the progress of this case I have had another strikingly similar. The patient is a gentleman in this neighbourhood, whom I have seen along with Dr J. Moir, in well-marked pneumonia of the lower lobe of the left lung, who had been only once cupped in the beginning of the disease, and in whom the inflammation, never very extensive, was attended throughout with so soft a pulse and so typhoid an aspect,—cold, clammy state of the skin, and sickness and vomiting,—that we thought it necessary to allow first wine and then brandy, omitting all depleting remedies, by the end of the first week of the disease; and have had the satisfaction of finding that the general symptoms have improved, and the inflammation gradually subsided, under this treatment.

And although we must confess much deficiency in our pathological information on this as on other subjects, yet it is easy to see a justification of the deviation of our practice in such cases of pneumonia, attended with typhoid symptoms, from the usual practice where the reaction is stronger and more permanent, if we merely attend to the *mode of fatal termination* which may be observed in these cases of typhoid pneumonia; for it may be clearly perceived in some of them that death takes place in a manner obviously distinct from the death in the more sthenic form of the disease;—the state of the circulation and of the nervous system, for some time before death, distinctly shows that the patient does not die asphyxiated, but exhausted, and often comatose, or nearly so,—in a manner resembling the usual fatal termination of typhus, rather than of any disease confined to the chest. And farther, on dissection in such cases, I have repeatedly seen the hepatized portion of the lung, although quite of the

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[1 In this case the improvement of the general symptoms continued progressive, and the sounds in the upper part of the left side of the chest went through changes corresponding very exactly to what is stated above as common, where there has been condensation, from any temporary cause, of the upper lobe; *i. e.*, we had dulness on percussion below the clavicle, and over the spine of the scapula, with bronchial respiration, resonance of voice, and subcrepitous râle, changing to mucous, and then to a sound not to be distinguished from cavernous; so that for some days the sounds were just those of advanced phthisis, but gradually the sound on percussion became clearer, the respiration more natural, and the râle dwindled away; and at this date (9th July) the sounds are nearly natural in these parts.]

usual appearance, so much less extensive than in other pneumonic cases, that the impediment to the arterialization of the blood thus presented could not be regarded as an adequate sole cause of death. We have no more precise mode of expressing this than by saying, that the shock given to the constitution by a given amount of inflammation in the lungs is greater and more dangerous in some persons, or in persons under certain circumstances, than in others. And that this should be so we can easily understand, because we know that the effects of the shock given by injuries of the whole body, as in cases of concussion, are very various in different persons; and again, what is still more applicable here, that in cases of peritonitis from cold, acting on a system previously healthy, where the sympathetic affection of the circulation is the immediate cause of death, the extent of inflammation which shows itself on dissection is extremely various. It is quite in accordance, therefore, with what is observed in the most analogous cases that we know, to suppose that the constitutional fever excited by pneumonia will be of a very different character, and imply different kinds of danger, in different cases.

Thus these observations on the peculiar form in which this well-known and important disease may present itself, tend only to an additional confirmation of a practical rule which is generally acknowledged, and which is indeed the main reason for our cultivating the pathology, as well as studying the nosology, of diseases,—although not perhaps so generally acted on as it should be: that we must, for the good of our patients, prescribe, not for the names of diseases, but for the nature and tendency of their most urgent symptoms. And in regard to the treatment of pneumonia, when attended by a kind of febrile reaction, in itself dangerous, and likely to be aggravated by depletion, the most important practical rule to be deduced from what has been stated is, not that blood-letting and other antiphlogistic remedies have no power over such inflammation, but that there is a danger involved in such inflammation which such remedies do not counteract, but after a time distinctly increase; and therefore, that it is peculiarly important in all such cases to recognise the inflammation, and apply that kind of treatment *as early as possible*, in order that it may be *as small as possible*, and *yet effective* in moderating the inflammatory process.

[A few days after this lecture was delivered, I saw, with Dr Dunsmure, another striking example of pneumonia, affecting the upper and middle lobe of the left lung, attended with unusual depression of the circulation, and particularly with so much nausea, vomiting, and faintness, that blood-letting appeared inadmissible in the beginning of the disease; and twelve leeches applied on the eighth day had no further effect than relieving the pain of side. This case was fatal on the eleventh day, and showed on dissection the usual appearance of hepatization and softening in nearly half the left lung. The case was further remarkable as an example of phthisis of the right lung, the symptoms of which had abated so completely under the use of the cod liver oil, four years before death, as to enable this man to carry on an extensive business as an auctioneer; and he had been exposed to cold, wet, and fatigue, in conducting a sale some miles from town, some days before the fatal attack. The uneasy sensations in the chest, during his fatal illness, were all referred to the left side, in which the crepitous râle, and the dulness on percussion were distinct; but it further appeared on dissection that two irregular cavities existed in the upper part of the right lung, quite empty at that time, and surrounded by small clusters of tubercles, all of them in the hardened, or nearly cretaceous state. The tubercular disease, therefore, had stopped short, after passing into ulceration; and considering what was observed as to other cases of pneumonia at the time, it could hardly be supposed that the peculiarity of the fever attending the fatal pneumonia of the left lung was connected with that inert diseased state of the right.—W. P. A.]

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